The Relation of Physiology to other Sciences.[1]

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PHYSIOLOGY AND THERAPEUTICS.

FROM the earliest times physiological knowledge, whether known by that name or not, has had the closest association with medicine. It would indeed be difficult to imagine any great advance in one that was not immediately reflected in the other. Their methods, though necessarily different, are convergent, their meeting-point being the disclosure of normal functions. It is the business of the physician to attend to the urgent call of pain and disease, and to use for their relief such information as he has at his disposal. As he does so he observes, compares, and draws conclusions on the basis of which a theory of the causation of the disorder may be built. The clinical observations and deductions drawn from them give a basis of rational physiological theory from which we have learnt that a state of disease is never a thing in itself, but is always a result of a quantitative change in some physiological process, an increase or diminution of something that was there to begin with.

No aspect of scientific activity is so generally misunderstood as that which concerns the making of discoveries, and in matters of medical research ignorance is particularly widespread. Discoveries are infrequent, in a sense fortuitous, and often dependent on rare qualities of intellect as well as on accurate observations, and they mostly come out of the fullness of time. We all feel great pride in recalling that one of the greatest of all discoveries, which has recently been celebrated as the tercentenary of the publication of William Harvey's famous book "De Motu Cordis," was made in our own country. Here was a genuine revelation that put old facts in a new light.

Incidentally it has been claimed, with more audacity than insight, that experiments upon living animals serve no useful purpose, and it has even been pretended that Harvey had no need for such experiments in the classical researches which formed the foundations of physiology and gave reason to physic. Riolan, in advancing against Harvey the criticism that "it is a mockery to attempt to show the circulation in man by the study of brutes," was, as Gley has recently remarked, "already employing the argument, if it can be called one, which is encountered under the pen of the antivivisectionists of all times, and which illustrates the diuturnity of ignorance and folly."

Let anyone with sufficient acquaintance with physiology try to write an account of such of the main facts concerning the functions of the heart and of the circulation as are most valuable in medicine, without reference to any fact obtained directly or indirectly by animal experimentation, and he will find his essay a very sorry one indeed: for no doctor can use a stethoscope, feel a pulse, take a blood-pressure, administer a hypodermic, give an anæsthetic or a transfusion, perform any modern operations, or indeed take any steps in diagnosis, prognosis, or treatment, without utilising at every turn knowledge derived from the results of animal experimentation and obtainable in no other way.

The subject of pharmacology is very closely connected with physiology on one hand and with therapeutics on the other. Rational therapeutics, based on the results of pharmacological study, also will carry into the wards the spirit of true scientific investigation, and the provision of beds in some hospitals for the use of the professor of therapeutics is an indication that definite progress is being made in this direction. Such an advance has not come before it is needed. If the medical practitioner is to compete successfully with osteopaths, chiropracters, and other similar unqualified persons, he is most likely to do so by only prescribing treatment with proper scientific basis. He should be able to form some opinion with regard to the claims of advertisers of remedies who contribute so large a share towards his daily mail deliveries.

It is, in my opinion, quite impossible, and perhaps undesirable, at the present time to frame instruction in physiology so as adequately to equip the ordinary medical student to proceed directly to the prosecution of research in any of its branches; this can only be achieved by a further year or two of study of the subject, such as by a science course for an honours degree. One of the objects of instruction is to enable the latest results of physiological investigation to be utilised in the clinic, and it seems to me that one of the best ways for this to be effected is for some workers specially trained in physiological methods to enter the staff of clinical units where facilities for research work are at hand.

The opinion was at one time prevalent among many clinicians, that if their problems required the use of methods similar to those of experimental physiology, these should be farmed out to a physiologist, and although there are cases where this procedure may be followed with advantage, the rich harvest which has already been reaped by the importation of physiological knowledge and methods into, rather than the export of problems from, the clinic, is adequate justification for the former. It is in any case encouraging to note the present-day decline of the attitude that experimental investigation is work of a lower order, which can be put out like so much washing, for the employment of an inferior caste.

The close connexion which is now generally admitted between physiology and medicine was clearly foreseen by Claude Bernard in 1855. Medicine, he said, is a science, and physicians who describe it as an art injure it, because "they exalt a physician's personality by lowering the importance of science." "True experimenting physicians," he says, "should be no more perplexed at a patient's bedside than empirical physicians. They will make use of all the therapeutic means advised by

[1] From the presidential address to Section I (Physiology) of the British Association, delivered at Glasgow on Sept. 10.

empiricism ; only, instead of using them according to authority and with a confidence akin to superstition, they will administer them with that philosophic doubt which is appropriate to true experimenters."

Physiology takes its place as a science in proportion as its data are accurate and its principles fall into line with those in the other sciences. My great teacher Starling said that science has only one language, that of quantity, and but one argument, that of experiment. The qualitative observations of one generation tend to become quantitative at a later stage of development of a science, and the degree of development of a science can indeed to some extent be judged by the extent to which it falls into a scheme of the unity of science by giving results which are capable of mathematical treatment and of expression in broad general principles.

What has happened in physics and chemistry may be reasonably expected to happen in biology, so soon as it is able by improvement in the accuracy of its methods and by progress in the formulation of its problems to employ mathematics with profit in the manipulation of data and in the construction of those generalisations which are landmarks of progress in all the sciences ; indeed we are, I think, now witnessing the commencement of such a phase in the development of our own subject.

Mathematics and mathematical physics have been of considerable use to physiology in increasing the accuracy of its experimental data, and this in two ways. First, by bringing the accurate experimental and intellectual methods of physics to bear on the construction and use of the numerous physical instruments which it employs. It has been said by Prof. A. V. Hill, that many of the early investigations on muscle were in reality studies of the properties of levers, and it is certain that similar remarks apply to only too many investigations in which the properties of the apparatus used have not been suitably investigated.

Even when the apparatus at the disposal of the physiologist is unexceptionable, however, it is often the fact that, owing to the nature of the subject, results are not susceptible of repetition with the same ease and certainty as are those of chemical or physical experiments. The variability of the results is due in such cases to what are called accidental circumstances, a term which in reality means circumstances over which we have no control, owing either to our ignorance of their nature, or else to our inability to alter them. In those cases where further study provides methods of more fully understanding and therefore more adequately controlling these circumstances, valuable results follow almost at once.

Under the most favourable conditions, however, it has up to the present been usual to find a considerable unavoidable margin of variation in the results of many physiological experiments. By regarding these provisionally as 'chance' variations, considerable help may be obtained by the application of the theory of errors, based on the theory of probability.

Lastly, as a means for evolving generalisations out of experimental data, and of bringing these into relation with the generalisations of other branches of science, the use of mathematics is incontestable. One need only mention as examples the fresh outlook which has been provided for further investigation by the exact study of the data relative to the segregation and recombination of hereditary factors, the beautiful investigations of L. J. Henderson on the equilibria in the blood, the theoretical study of the phenomena of excitation, the employment of thermodynamics and the numerous other applications of physico-chemical theory.

Chemistry and physiology having both originally sprung from the art and practice of medicine, it is little matter for surprise that such a rich harvest has been reaped by their reunion in the form of biochemistry. Although these developments were foreshadowed by the intuition, if not by the actual achievements, of the iatro-chemists of the sixteenth century, little advance was possible until chemistry had, by separation from medicine, established its position as an independent science. So that it was not until about 1840 that organic chemistry and biochemistry were able, chiefly owing to the inspiration of Liebig, to make rapid progress, at least on the Continent. It is significant that at the present time a steadily increasing number of young highly trained organic chemists consider it worth their while to turn to biochemistry ; their welcome entry into our ranks gives us fresh hope and faith in our future, as well as in theirs.

As is usually the case, rapid developments in biochemistry have followed improvements of technique ; the advances in micro-methods of analysis, without which insulin would probably not have been discovered, or the constitution of thyroxin made known, have played a very important part ; the same applies to the whole subject of physical chemistry, much of which, like colloid chemistry and the theories of buffer action, has been built up in response to biochemical requirements. Since the central problems of biochemistry are dynamical, most of its subject matter must be treated from that point of view, and here again the debt to physical chemistry must be recognised.

Whether a biochemist should be primarily a chemist or a biologist is a question which has been much debated in private, though little in public. Personally I see no reason why he should not be both. If he must have one label, it is better that of the chemist, provided always that the biochemist works in the closest possible association with the physiologist. In fact, I am convinced that within the limits of administrative possibility, the greater the variety of workers brought together the better the results.

So much for the exact sciences. Their value to physiology is immense. They help us to interpret phenomena, but not to predict. In a word, physiology is something more than biochemistry and biophysics ; it is, and will always remain, a biological subject.

As its nearest neighbour among the biological sciences, zoology should have the closest relations with physiology, yet it is curious that during several decades, for reasons which need not now be discussed, these two subjects were as the poles apart. The newly disinterred subject of comparative physiology, however, bears witness to a returning interest of zoologists in the experimental study of function as against mere morphological classification, as well as of physiologists in comparative function as a valuable means of throwing light on their own special problems.

The relation of anatomy to physiology can best be understood if we recall the fact that when the time was ripe physiology separated off from anatomy, taking with it all those dynamic problems which concerned function, and leaving anatomy literally little but the dry bones. The stationary condition of anatomy during the last decades of the nineteenth century was similar to that of zoology, and indeed had similar causes, and was little relieved by the subsequent incorporation of anthropology and embryology.

Histology had in most countries remained with anatomy, and had for the most part been content, like it, merely to describe the structure of preserved dead things. In Britain, it is true, histology had until quite recently everywhere remained with physiology, and had perhaps fared no better, for although the British, like their Continental friends, did 'nothing in particular,' they did not do it very well, for we must admit that histology had degenerated into a merely descriptive subject, supplemented by training in a useful technique, and by the identification of specimens. Nevertheless there were rays of hope, and occasional hints, that the problems of function had not been entirely lost sight of, and that the large mass of histological information which had been collected might become valuable if only the fundamental question as to the reality of the structures described could be settled.

At the present time some English schools have followed the American and Continental practice, and handed histology over to anatomy and though I am personally not at all convinced of the justification of this step, yet in view of the indications of quickening in the subject of anatomy during the past two decades, it no doubt is best to suspend judgment as to the ultimate result of the transfer.

I have, I hope, said enough to lend emphasis to my principal point, which is that the subject of physiology has the most intimate and vital contact with all biological subjects, with the fundamental sciences, and with medicine. It is, in fact, one of the best possible illustrations of Herbert Spencer's idea that "the sciences are arts to one another." It has often been said that science knows no frontiers and no nationalities. If we apply this a little nearer home, we shall all look forward to the day when departments will merely indicate administrative boundaries and not intellectual compartments.

Although the application of those sciences which are called 'exact' is of immense value to physiology, we must be under no misapprehension as to their real relation, which is merely that they enable the phenomena of life to be described more accurately. They in no way furnish an explanation of those phenomena or enable us, without direct reference to physiological facts, to forecast them. The so-called exact sciences appear to be so because of the simplifications of which they are capable, by reason of which problems can readily be formulated and attacked. Disturbing conditions can provisionally be ignored or allowed for, and a first approximation reached which can be corrected later. In biology this can less readily be done. It is the failure to appreciate this elementary fact which leads some of those trained only in the methods of the exact sciences into the most palpable and unpardonable blunders when they attack biological problems.

The process of application of the exact sciences to physiology consists in reality of studying the phenomena themselves and then adopting the most plausible explanation capable of formulation in terms of the exact science. There is no other way. But let us be under no illusion about finding final explanations of what life is by this or any other methods.

It was pointed out long ago by Claude Bernard that all *a priori* definitions of life, like those of time, space, or matter, are futile, since they usually themselves imply the thing defined. Let us take one or two famous definitions of life as examples. Bichat in 1818 defined life as "the sum total of those functions which resist death." Here we have two opposed ideas, life and death. "All that lives will die; all that is dead has lived." For Bichat, life is a struggle of the living thing against an environment which seeks to destroy it, but it is clear that the idea of life as opposed to death is implicit in the definition. This idea of an internal teleological principle, of entelechy, runs through all biological writings back to Aristotle, with whom we believe it to have originated. The amœba which encysts itself does so in order to defy adverse conditions in its environment. The 'calculating intelligence' postulated by Kant directs this response.

Another definition of life which has been much favoured of late is the mechanistic one in various forms; 'life is a special activity of organised things.' Here again the definition implies the idea itself. The possession and maintenance of a definite structure cannot any longer be held to be an outstanding feature of living matter as commonly understood, for recent researches in physics show us that, although electrons may come and go, the atomic structure of matter is relatively stable, even though in particular circumstances mutations may occur. Nevertheless, the view of life as a mechanism created by and entirely dependent upon its environment gained strength owing to the developments in other sciences, particularly by reason of the synthesis of organic compounds, the principle of the conservation of energy and the introduction of the Darwinian theory of evolution. According to this view, a revival of that of Empedocles, teleological manifestations are accidental. As that thoughtful writer Hjort remarks, however: "When we, as human beings, call a thing accidental, it only means that we give up the hope of understanding it. . . ." "In the physical sciences those factors are termed accidental which we voluntarily disregard in the course of an investigation, or which we find we have omitted to notice." Kant, however, in his "Kritik of Judgment" calls the teleological "the link whereby our understanding can alone be supposed to find any agreement between the laws of Nature and our own power of judgment."

Mechanistic interpretations tend in the long run to become arrogant and superficial, as vitalistic ones predispose to scientific nihilism. For, while it is inconceivable that living things do not obey the laws of Nature, yet it is equally unthinkable that a chance encounter of physico-chemical phenomena can be the explanation of their existence. This being so, how can we, in Kant's words, "arrive at an understanding of Nature"?

It seems clearly impossible to harmonise or to decide between these opposed views of the nature of life, and I do not think any final conclusion to be possible or even necessary. To quote Hjort once more, "Philosophy has no other starting-point than a problem, and the current results of scientific research; it never leads to any absolute conclusion. It grows with the science of Nature, since in reality it comprises the most general results of that science and comprises nothing more. It does not explain the nature of the human understanding, and provides no means of getting behind the understanding itself . . . the existence of which is the first and necessary condition for the existence of science at all."

Physiologists, in attempting to know what life is, have in my opinion attempted too much, and I think that a new point of view is essential. One of the greatest of contemporary thinkers, L. J. Henderson, has recently submitted an argument with which I venture humbly to agree. The idea of adaptation, urged by Claude Bernard, should be adopted by physiology as its basal principle, as the chemist accepts the conservation of matter, or the physicist the conservation of energy. We need not seek to know why it is so: that is the province of the philosopher; all our experience tells us that it is so. It is not a definition of what life is, but a brief statement of its way, which is valuable, stimulating, and true. But we must treat the organism and its environment as one if we are to gain a proper insight into the adaptations manifested by the former. Life is conserved by adaptation, and I think that this conception will be useful alike to general biology, to physiology and perhaps most of all to pathology.

It is the concern of physiology to study the normal functions, and here the normal must be regarded as a statistical group. For particular purposes it is convenient to consider normals as of fixed value; but for other purposes it is equally convenient to regard each of these in turn as variable, to study its variations and find how they are produced. When we do so, we find, with increasing clearness the more deeply the subject is investigated, that the variability and the constancy are closely related, the fixed value of one thing being due to the interplay of the variables of others.

We have in the study of physiology many beautiful examples of this closely woven texture of interdependent phenomena. Modify any condition concerning any one of them, and we at once set the machinery moving in such a way as to counteract what we have done; and this is not what life is, but what it does, which distinguishes it—it adjusts the organism to its environment.

Glancing now towards the future, what may we say represents in a few words the trend of modern physiology? In many ways a great future lies before it. Utilising the other sciences as its tools and itself reacting powerfully on them, we can confidently predict progress to undreamt-of heights, an enormous development of experimental pathology and medicine, and far-reaching effects on economic and sociological conditions. Yet, implicit in these very potentialities, there is another and a gloomier side to the picture. The rapidly accumulating wealth of detailed knowledge and of special technique demands an increased specialisation; unless there is a periodic intellectual stock-taking, there must inevitably be a loss of perspective and of grasp of great general principles.

The establishment of special research professorships, however profitable in isolated cases, cannot in my opinion make good this growing specialisation, because it will tend to divorce research and teaching and place the teaching professor on a level of real or apparent inferiority. The idolisation of research for the sake of the advancement it brings is another of the dangers which threaten us. If there is one thing worse than 'a mediocrity who does no research' it is 'a mediocrity who does.'

There are at the present time a large number of junior research posts available, but not enough well-trained people adequately to fill them. This is all to the good provided that those who on trial show no aptitude for the work can be ruthlessly eliminated. As they often cannot, there are in consequence a number of young people who drift from one research scholarship to another, perhaps not aimlessly, but with no better objective than the manufacture of papers designed to justify their employment. The hapless editors of each of the swelling tide of journals are coaxed, hoodwinked, and, if necessary, bullied, to ensure that these papers see the light of day. In the fullness of time the list of short-time research posts is exhausted, and the young investigator must now either turn to some entirely different occupation or else, as one of my friends expressed it, 'subside into a professorial chair' for which, incidentally, he is probably entirely unfitted.

The pursuit of science is nowadays, perhaps unfortunately, a career, and one in which moreover it pays to advertise. Science, we are often told, is the cream of civilisation. If we believe this, let us use all our endeavours to ensure that it be not a whipped cream, specious, puffed up with wind, and presenting a fictitious appearance of solidity.